

menting gave more striking results than that proposed by Askenasy, who kept the hyacinths in the dark until the whole inflorescence had opened, then cut off the upper part of the flower-stalk with half of the flowers and exposed it to daylight, while the others remained in their dark place. But he also saw that the latter flowers remained less coloured.

Experiments were also made by me with *Aucuba japonica*, which plant produced white flowers in the dark instead of purplish-brown ones in the light, just as the lilac does when cultivated in winter by florists. On the other hand, flowers of *Crocus vernus* and *Tulipa gesneriana* did not perceptibly change their colour. Among the leaves I experimented upon, I mention a *Coleus* which in the light produced green leaves with red lines, whereas the same sort gave in the dark yellow leaves with almost colourless lines. *Achyranthes* behaved very curiously, producing in the dark two normally coloured leaves, but the new-formed internode which supported them was almost white instead of red. This result calls to mind that of Batalin, who found ("Acta horti petropolitani," t. vi.) that plants of *Polygonum Fagopyrum*, which he had raised from seeds in the dark, were quite colourless, so that the difference from those grown in the light is very great, the latter, as is generally known, being of a dark red colour.

It appears from these examples, that those organs which are put into the dark in a very undeveloped state (so that they must much enlarge there) undergo much discolouring, whereas those parts that are hidden in a more advanced stage of growth, lose less colour, in some cases almost none.

Lilacs, for instance, develop in the dark from quite small studs, and so do flowers of *Aucuba*. Buck-wheat plants grow from small seeds containing a small hypocotyl, that enlarges afterwards to an exceedingly long part. The above-mentioned colourless long internode of *Achyranthes* came from a very small stud. In all these cases there can be only a small quantity of pigment (or chromogene) in the part before its development, and only this small quantity seems to be spread over the same part when many times enlarged. These cases, accordingly, render it very probable that light is necessary to increase the quantity of pigment, and that the pigment present, originates from the time when the plant was exposed to sunlight.

But tulips, hyacinths, crocuses, berries of *Aucuba*, &c. lose very little of their colour in the dark. Why is this? Because the buds of the flowers named, when hidden in the bulb are quite complete, and the green fruit of *Aucuba* had reached nearly their natural size at the time they were deprived of light. I think these flowers and fruits had already stored up a great deal of chromogene which they received from the leaves during the time when the bulb and the fruit were formed, just as they possessed a sufficient quantity of food to reach perfect development of all parts in the dark.

According to this opinion, the colour of a flower, fruit, leaf, &c. when grown in the dark, depends only on the quantity of colour-making matter or chromogene that is contained in the part at the moment when it is withdrawn from the light. This opinion is supported by an observation of Askenasy's (*Bot. Ztg., L.c.*), who found that buds of *Pulmonaria officinalis* in different stages of development, when placed into the dark, got their colours the more perfectly the larger the buds were at the time of darkening. The buds which were smallest at that time exhibited almost white flowers.

My conclusion is then, that light is necessary for forming, if not the colouring matter itself, yet a matter (chromogene) which can easily pass into the pigment.

J. C. COSTERUS

Amsterdam, March 12

The Electrical Resistance of Carbon under Pressure

I AM indebted to Mr. Herbert Tomlinson for drawing my attention to his most interesting comparisons between the behaviour of carbon and of metals in respect of change of electric resistance under mechanical stress, and am glad to see that his delicate determinations entirely support my conclusion that the excessively slight change of specific conductivity produced by stress in hard coke carbon cannot possibly explain the great variations of resistance observed in the carbon telephone, carbon rheostat, &c. I have also learned that a similar conclusion was arrived at some time ago by Professors Naccari and Pagliani, and that some experiments made by Prof. W. F. Barrett of Dublin on the buttons of compressed lamp-black prepared in Edison's laboratory for use in his well-known carbon tele-

phone lead to a precisely similar result. I am therefore perfectly willing to admit that before the publication of my experiments this question was virtually settled. It is quite clear that the carbon telephone does not work by any variation in the specific resistance of the carbon, but by the partial opening and closing of the circuit at certain surfaces where the intimacy of the contact can be varied by the vibrations.

Bristol, March 20

SILVANUS P. THOMPSON

Vivisection

As I am named in your article thus headed (p. 429), I shall be obliged if you will permit me a brief explanation of my position in regard to the question. There is doubtless in point of suffering a very great difference between such experiments on animals as those tabulated by Prof. Yeo (and cited in your article) and those common upon the Continent, and of which the horrible tortures perpetrated by Professors Schiff, Mantegazza, and Paul Bert may be taken as fair types. But I fail to see by what means, whether legislative or otherwise, the atrocities of vivisection are to be prevented, while its more moderate practices are to be permitted. Had the sins of the experimentalists been confined to the "hypodermic injection" of a few mice, as Sir James Paget has tried hard to make the public believe, I venture to assert that no outcry would ever have been raised on the subject. I for one should certainly have thought the cause unworthy of support. But the truth is, unfortunately, far otherwise. It is not against the inoculation of "vermin," or the pricking of a "tadpole's tail," that the indignation of the English people has been stirred, but against the prolonged and exquisite tortments to which such highly sentient creatures as horses, dogs, and other domestic animals have been and still are subjected, often under the influence, not of chloroform, but of curare. So long as English physiologists continue to cite these things with approval, and openly to regret that the effects of public opinion in this country are such as to throw obstacles in the way of "free" and unlimited vivisection, as practised in the Continental schools, so long, I apprehend, the agitation deprecated in your article, will go on, and gather strength. It is the influence of public opinion only, which in this country finds expression as it does in no other, that has hitherto prevented the physiological laboratory of England from becoming as notorious a scene of horror as that of France, Italy, or Germany. And, let it be borne in mind, that the stir on this subject, which has expressed itself recently in the pages of various reviews and magazines, was set going by the demand of the Medical Congress for *unrestricted* vivisection. Sir William Gull's opening sentence in the *Nineteenth Century* of this month is, therefore, wide of the mark. Not less does he mis-state the case, when, in the course of his concluding observations, he infers that I bring against physiological research the charge of Atheistic tendency. That charge is distinctly made by me, not against legitimate research of any kind, but against a method which I wholly dissociate from "science" properly so called. And it is a charge not lightly made, but based on sound experience and thoughtful observation, unbiassed by "emotion" of any kind.

As it has not been my privilege to escape the customary personal retort of pro-vivisectionists with regard to the wearing of fur, feathers, and the utilisation of animals, whether for food, pleasure, or clothing, I shall hold myself indebted to your courtesy for permission to make the following statement in justification of my own consistency:—I never buy furs, feathers, ivory, kid gloves, stuffed birds or other creatures. I have long been engaged in trying to introduce the use of vegetable leather for making boots and shoes, and have devoted much of my time to the question. I detest all "sport" which necessitates the pain and suffering of living creatures, and have written many articles and letters to various journals for several years past against seal-hunting, pigeon-shooting, coursing, battues, and rabbit-gins. Of late years I have added "vivisection" to the list. My husband's horses wear no bearing-reins; and I never see cruelty without interfering at the risk—as I know but too well—of personal insult. Finally, it is twelve years since I tasted flesh or fowl of any kind.

ANNA KINGSFORD

11, Chapel Street, Park Lane, W., March 16

[It seems a somewhat unwarranted and unworthy argument that because some foreign experimenters have been guilty of excess, therefore we are totally to suppress in England that which all competent persons know and declare to constitute a most im-

portant branch of physiological method; and there is certainly no ground for the statement that it is "the influence of public opinion only" which has hitherto prevented abuse in this country. As pointed out in our article, the English physiologists themselves took the initiative in laying down a code of rules against abuse some time before the sense of the general public was aroused, and it does not appear that since this has been aroused there has been any change in our physiological practices. Moreover, we cannot think that many of our readers will resemble our correspondent in failing to see that "atrocities" may be guarded against while "more moderate practices" are allowed; and in this connection it is always to be remembered that our English physiologists have gladly acquiesced in legislation directed against the former.—ED.]

TOWARDS the close of an interesting article on Vivisection in a recent number of NATURE (p. 429), the following remarks from an article in the *Fortnightly Review* by Lord Coleridge occur: "What would our Lord have said, what looks would He have bent upon a chamber filled with unoffending creatures which He loves, dying under torture deliberately and intentionally inflicted." Prof. Yeo in answering this, quotes "Ye are of more value than many sparrows," "How much then is a man better than a sheep." But there is one passage in Scripture which I think has even a closer connection with vivisection than those mentioned above, namely, the healing of the man possessed with devils (Mark v. verse 13), "and the unclean spirits went out and entered into the swine; and the herd ran violently down a steep place into the sea (they were about two thousand;) and were choked in the sea." If our Lord therefore considered it expedient to permit the destruction of a whole herd of swine, numbering 2000, in order to alleviate the sufferings of the demoniac, surely the labours of a man like Hunter must be justified, who by experiments on living animals has been the means of reducing death from aneurism of the principal artery of the lower limb from 95 per cent. to 10 per cent., as stated by Sir James Paget.

CHARLES ALEX. STEVENSON

45, Melville Street, Edinburgh, March 14

Muffs and Vivisection

YOUR correspondent, Mr. H. H. Johnston, of the Zoological Gardens, has offered your readers some "facts," which, he says, he "knows to be true." He says that some little time ago I called on "a distinguished member of science"; that "three things were observable in my outward presentments"—to wit, ostrich feathers in my bonnet, a bird of paradise on, or near my muff, and an ivory-handled umbrella; and that the man of science took each of these articles as a text for a rebuke to me for encouraging cruelty. Sir, these "facts" may possibly be "accurate enough for scientific purposes," like some others which we heard of at Bond Street, last winter, but they have given much merit to those who happen to be acquainted with my real "outward presentments." Suffice it to say, that I never paid such a visit as Mr. Johnston describes; never received such a rebuke; never used an ivory-handled umbrella; never wore a bird of paradise, or any other bird, either in or near my muff, or any other portion of my attire; and, finally, having never possessed such an object in my whole life, am driven to think that the only Muff connected with the ridiculous story, must be the person who assures us he "knows" it to be true.

1, Victoria Street, S.W.

FRANCES POWER COBBE

Pasteur's Inoculations

In the *Proceedings* of the St. Louis Medical Society, recorded in the *St. Louis Medical and Surgical Journal* for December last, Dr. Spinzig is reported to have said:—"Splenic fever only prevails where there is low ground, or what is called 'bottom ground.' Sheep that are fed and shepherded on such ground will take this disease, while those that are shepherded in lofty regions never take it. But Prof. Pasteur had perfect confidence in his antidote, and asserted that if the sheep were properly 'vaccinated' it would make no difference where they were, because the specific poison could not reach them. In Germany, however, they found this was not the case." May I ask, through your columns, what is Dr. Spinzig's authority for this statement?

Again, I have been informed, that although it has been abundantly proved that cattle and sheep inoculated with attenuated virus are protected against the otherwise deadly effects of uncultivated virus, they do not enjoy similar immunity from the ravages of a natural epidemic of splenic fever. I should be glad to know whether any record is forthcoming as evidence that the inoculation of the attenuated virus has proved as successful against natural epidemic arising under ordinary conditions as against artificial introduction of the virulent matter into the system.

A STUDENT

"Eophyton"

AFTER all that has been written, both in England and abroad, about the inorganic nature of Eophyton, it is strange to see such statements as those made by Mr. Elsdon, in NATURE (vol. xxv. p. 409). As long ago as 1873, I pointed out that in Sweden it occurs under circumstances, which most decidedly prove that it cannot have been any plant or organic being whatever, and Principal Dawson has been led to precisely the same conclusions, from his studies of Eophyton at St. John. Last year, I further, by way of experiment, produced Eophytons, which cannot be distinguished from the Cambrian ones.

A. G. NATHORST

Geological Survey Office, Stockholm, Sweden, March 16

"Telescopic Definition in a Hazy Sky"

MR. ROYSTON-PIGOTT's letter in NATURE, vol. xxv. p. 77, reminds me that during the Transit of Venus in 1874, the sky here for the greater part of the day, being covered with thick but varying clouds, I several times, for a considerable time together, saw the planet on the sun's disk, with the naked eye; the cloud at these times stopping out the superfluous light, but not stopping distinct vision.

I may add (though off the line) that, towards evening, the sun shone out brilliantly, and from my garden, through a bit of smoked glass, I watched the whole egress. Unfortunately, the subsidiary station, which was to have been here, had to be abandoned at the last moment, a matter of great regret to all concerned, and to all interested, and not the least to myself, having witnessed a great chance which there was no one to use.

Nelson, New Zealand, January 27

A. S. ATKINSON

The Weather in South-Australia

MRS. MERRIFIELD, with her compliments, begs to inclose extracts from two letters she has recently received, describing the hot weather in South Australia during last January, also mentioning the curious fact, that the water in the Sturt river is more abundant in dry seasons than in wet ones. Perhaps the Editor may consider the statements sufficiently interesting for insertion in NATURE.

Stapleford, near Cambridge, March 14

"On Tuesday, January 17, the thermometer was registered at 180° in the sun, and 114° in the shade. I believe we had it hotter here; for, in the afternoon, the hills were covered with bush-fires. Fortunately, they were on the other side of the railroad, about two miles off. At night, when the fire was nearly out, and only the stumps and dead branches left burning, it looked like a large town lighted up. A strong south-east wind blew the heat over to us. In the morning, a strong hot blast came from the north, as hot as I have felt it in West Australia. Yesterday, the thermometer fell, out of doors, to 90°. To-day (January 19) was, I think, as hot in doors as on Tuesday. Metals in the room were unpleasantly hot to the back of the hand. The leaves are falling off the trees, from the intense heat and dryness of the air. The ants, which up to 9 a.m. were busy, forming a column varying in width from eighteen inches to three feet, and almost colouring the ground, retired to their nests. Not one was to be seen. Neither a bird, fly, nor other insect was visible, unless disturbed by the rustling of the leaves. The following statement will show the hottest days in certain years:—

In 1860	...	thermometer	158
1862	...	"	159
1871	...	"	153
1880	...	"	172
1882	...	"	180

"February 2

"It is a curious circumstance that the Sturt river or creek, which runs through Mr. L.'s garden, has more water in it in dry